SCIENCE

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THE CLASSIFICATION OF NERVOUS REACTIONS[1]

IT is within the memory of most of us what a distinct advance was made in the definiteness of our thinking about nervous reactions when the introduction and improvement of the Golgi method led up to the conception of the neurone doctrine. Previous to that time our mental picture of the reflex mechanism was not essentially incorrect; but its conception of the nature of the connection between the sensory fiber and the motor nerve cell was indefinite. When the new histological method revealed the posterior root fibers entering the cord and by means of collaterals ending in the immediate neighborhood of motor cells, there was revealed an almost diagrammatic mechanism which explained many reflex phenomena; and we can recall the enthusiasm with which all proceeded to construct combinations of neurones to serve as the anatomical basis of the various known functions of the nervous system; indeed, we have been engaged in this fascinating pastime ever since.

This is exactly as it should be, for only in this way could the possibilities of the new discovery be tried out. There is danger, however, in anything which is attractively definite; sometimes because it may belong among those things which are "too good to be true"; but more frequently because its successful explanation of many of the phenomena with which it deals may blind us to its failure to explain others;

[1] Address of the chairman and vice-president of Section K (Physiology and Experimental Medicine), American Association for the Advancement of Science, Philadelphia, December 31, 1914.

and I think there are to-day, even among those who do not follow Apathy and Bethe in their undiscriminating attacks on the neurone theory, many who are seriously asking whether the neurone conception of the reflex exhausts the possibilities or nervous mechanisms.

For, after all, the distinct service of the neurone theory is its explanation of the mechanism of reflex action. It gives us a satisfactory explanation of simple and even of highly coordinated reflexes; but there are still left problems upon which its staunchest defenders will not claim that it throws much light. The contribution, for example, of the cerebellum to the execution of volitional or reflex actions is not self-evident in terms of the theory; the mechanisms of reenforcement and facilitation (Bahnung) are no more easily pictured now than before; and, above all, the whole field of cerebral physiology may be said still to be in the same state as was that of reflex action before Golgi made that fortunate mistake of putting some pieces of spinal cord which had been hardened in Müller's fluid into silver nitrate and beheld for the first time a nerve cell in all its glory.

The diagrammatic clearness of the picture of the reflex mechanism thus revealed has contributed largely to our present mental approach to the problems of neurology, an approach which is faithfully reflected in our text-book presentations of the subject. The first text-book chapter is largely anatomical, chiefly histological; the next chapter deals with reflex action which is presented to the student as par excellence the typical nervous action; the treatment of the subject then proceeds from the simpler reflexes to those requiring a higher degree of coordination; accustomed movements, such as those of locomotion, receive their explanation as an endless chain of reflexes requiring for its operation the structures of the mid- and 'tween brains; finally the attempt to explain everything in terms of reflex action is carried into the field of cerebral physiology. With what success? One has only to read the text-books to find out. Some things, especially localization, are dwelt upon at length; the possibilities of excessively complex coordinations are suggested by the anatomical structure; but we miss entirely the satisfaction of seeing the cerebral functions clearly pictured in terms of neurone structure. We trace the "way in" and the "way out"; we see that the connection between the afferent and efferent nerve fibers is in the cortex; but what takes place in the cortex? Is it objectively nothing more than our typical reflex raised to the *n*th power of complexity? Perhaps it is; but does any one feel reasonably sure of it? For one, I confess I do not.

However that may be—and I have no intention of discussing the question—this much may certainly be said. We know that there are nervous actions which are not reflexes at all; furthermore, there are nervous actions which usually pass as reflexes, although they present striking and perhaps fundamental points of difference from the typical reflex arc of our neurone theory. The justification of these statements will be attempted in what follows. My present purpose and indeed the purpose of this paper is to challenge the wisdom of making the reflex arc the type of all nervous action either in our own thinking or in the presentation of the subject to students, and to suggest that we would act more wisely to cultivate a more open state of mind with regard to the existence of other possibilities.

This may be done, it seems to me, by drawing sharply the distinction at the outset between the following classes of nervous

action: automatic, axon reflex, unconditioned reflex, conditioned reflex and volitional. It is not claimed that this list is exhaustive. The physiology of such mechanisms, for example, as the plexus of Auerbach is at present too little understood to admit of successful classification. It is only claimed that the above are distinct forms of nervous activity; that they are carried out by different mechanisms, and that, as such, they should be given coordinate rank in the student's mind.

In the following discussion I shall not include any treatment of volitional action. I am concerned only with the proper classification of nervous actions and see no reason for changing the all but universal custom of placing volitional actions in a class by themselves. I shall, however, dwell at some length upon the automatic action, the axon reflex, and then discuss together the conditioned and the unconditioned reflex.

AUTOMATIC ACTIONS

In general an automatic action is one which originates in the mechanism involved and is not caused by any external influence acting only at the time of its occurrence. The ticking of a clock is an example. In the field of physiology we think at once of the beat of the heart, although other no less striking examples are known; a strip of the muscular coat of the stomach or intestine shows automatic contractions; and many of the processes of embryological development probably belong in the same category. Contrasted with these are skeletal muscles and many glands which become functionally active only in response to some sort of external stimulus, usually a nerve impulse.

Even in the case of the skeletal muscle, however, the external stimulus seems to act by causing the accumulation, and probably the localized accumulation of some physical or chemical condition within the cell leading to the discharge of energy. Thus an attractive theory of electrical stimulation of skeletal muscle supposes that certain semipermeable membranes within the muscle fiber are more permeable to ions of one electrical charge than to those of the opposite charge; hence in the migration of the charges to the two electrodes during the passage of a current there results an accumulation of electric charges at these membranes; and when this has gone on to a certain extent, the electrical condition thus created explodes an unstable fuel substance, energy is liberated and contraction results. The passage of the stimulating current has merely produced the accumulation of what may be called the "discharging conditions" within the cell.

In the automatic action these discharging conditions seem to accumulate without external assistance, possibly as the result of certain metabolic processes in the cell itself. External conditions, such as temperature, may influence the rate or the amount of discharge; but this does not make these external conditions stimuli in any true sense. Furthermore, we may speak, if we will, of the whole chain of events leading to the accumulation of the discharging conditions as an "inner stimulus"; but this would seem to involve an unnecessary and even questionable extension of the term stimulus.

An automatic nervous action is frequently defined as a discharge from a nerve cell caused by some other external stimulus than that of an exciting neurone; but, if the cell is discharged by an external stimulus of any kind, the action is not automatic. What we observe in such cases is activity, apparently, at any rate, arising within the cell itself, and we have no more

logical right to assume an unrecognized stimulus than in the case of the ticking of a clock. In the absence of knowledge to the contrary, the presumption is that the cell is discharged by the operation of its own never ceasing metabolism.

In our usual teaching as well as in our usual thinking it is customary to take account only of (1) the stimulus and (2) the reaction, and to regard each of these as a single process; whereas all the evidence goes to show that between the ordinary external stimulus, at any rate, and the release of energy there is usually interpolated a third process, which we have termed the "accumulation of discharging conditions" in our brief reference to the nature of electrical stimulation. We again meet with the suggestion of a similar process in the case of stimulation by a nerve impulse. Langley's work on the antagonism of nicotin and curare, as well as that of Keith Lucas on the "characteristic" of stimulation in different tissues, has led to the assumption of a "receptive substance" in skeletal muscle. The action of adrenalin also points to a similar "receptive substance" connected with the endings of the post-ganglionic automatic neurones. And yet is not this term "receptive substance" or "receptor" merely a name to hide our ignorance? and do we not really mean a physical or chemical process carried out by the cell protoplasm, as a whole, rather than a specialized irritable substance; a process, in other words, which results in the accumulation of "discharging conditions"? If this point of view is correct we must distinguish sharply between the stimulated action and the automatic action; they are alike in the second and third of the above-mentioned processes; they differ in that the accumulation of discharging conditions comes in the stimulated action as the result of an external influence (electric shock, nerve impulse, or mechanical blow), while in the automatic action it results presumably from the cell metabolism.

In the central nervous system the best known and most successfully studied case of automatism is that of the respiratory center. The conclusion which Rosenthal drew from his experiments, that the nerve cells of this center send out rhythmic discharges when removed from all connection with afferent nerves, has been confirmed by all subsequent work, the experiments of Winterstein being especially conclusive on this point. Here again we are probably dealing with the development within the nerve cell of discharging conditions which may be influenced by the character of the environment, such as the tension of carbon dioxid or the concentration of hydrogen ions, or temperature or the presence of certain drugs; and we may repeat that there is no justification for speaking of these as stimuli, as we generally do. So far as the facts go, we may logically regard them only as external conditions which regulate the rate of development of the automatic cell processes or the character of the discharge which it evokes from the cell.

Until recently no other case of automatic nervous action was known. Some may have been suspected in the vaso-motor system or in the myenteric plexus; but no facts compelled the conclusion that they must be regarded as automatic actions. Recently, however, facts have come to light which argue strongly for an automatic basis to the nervous mechanism of locomotion. These movements, as already stated, have in the past presented to us the picture of an endless chain of reflexes, in which the afferent neurones are mostly the nerves of muscular sense, the complex of instreaming afferent impulses, ever changing as the movement proceeds, giving the

appropriate stimuli to the successive movements, which eventually come back to the starting point and so lead to the repetition of the series. To avoid misunderstanding, it may be well to say at the outset that no one denies that afferent impulses play an important rôle in locomotion. The phenomena of locomotor ataxia are conclusive evidence on that point; but so do afferent impulses over the pulmonary fibers of the vagus nerve play an important rôle in regulating the fundamentally automatic discharge from the respiratory center, without being in any way its exciting cause. The work of T. Graham Brown[2] suggests that the same thing is true of the rhythmic movements of locomotion. Brown shows that in a certain stage of ether narcosis in the decerebrate animal, when reflexes can no longer be elicited from the afferent nerves, rhythmic movements of flexion and extension occur in the hind legs; and furthermore, that these movements occur after the afferent nerves from the moving limbs are cut. In other words, these movements which suggest the basis of the movements of locomotion, involving as they do the alternate rhythmic action of antagonistic groups of muscles, are executed by efferent neurones without any stimulation from afferent neurones. They constitute an "endless chain," but not an endless chain of reflexes.

This discovery seems to me to be of sufficient importance to justify dwelling upon it at some length; and in order to obtain a clear picture of the possibilities, we may give briefly Brown's very plausible hypothesis of the nature of the nervous mechanism involved. A movement of this kind consists fundamentally in the alternate contraction of antagonistic groups of muscles. We may denominate the nerve cells innervating each antagonistic group as a half-center, the two together making the entire nerve center for the given movement. Brown supposes that each half-center sends inhibiting collaterals to its antagonist (reciprocal innervation of Sherrington), so that when the flexors, for example, are being excited, the extensor neuro-muscular mechanism is inhibited. He then assumes that the efficiency of this inhibition rapidly diminishes—somewhat as the heart escapes from vagus inhibition —either by the fatigue of the inhibitory mechanism or by the increase of the discharging power of the inhibited cells. The result is that in a short time the inhibited center breaks through its inhibition, excites its muscles to contract and at the same time inhibits the previously active antagonistic half-center.[3] The repetition of these processes leads, of course, to the rhythmic movements referred to.

Brown further raises the very interesting question whether these automatic actions of locomotion do not present a more primitive form of nervous activity than the reflex. He points out the difficulty of imagining the origin of a reflex arc by natural selection, since neither the afferent nor the efferent limb would be of any use to the animal without the other; and it is almost impossible to conceive of both arising at the same time by any assumed process of evolution. It is far more easy to

[2] T. Graham Brown, *Journal of Physiology*, 1914, XLVII., p. 18.

[3] This theory assumes that the cells of both half-centers are automatic and subject to the same environmental conditions (*e. g.*, tension of carbon dioxid) governing their discharge. If both centers were in exactly the same physiological condition and subject to the same environmental conditions, they would discharge simultaneously and alternate rhythmic contractions of antagonists would be impossible. This condition of equal irritability, however, is rarely realized. When it does not obtain, one half-center will discharge first and, as explained, inhibit for the time being the discharge of the other.

suppose that the primitive nervous mechanism is the automatic one seen at work in the movements of narcosis progression. These would serve in the simplest animals the purpose of progression which may reasonably be regarded as among the first functions of coordination a nervous system would be likely to serve. In other words, the nervous mechanism of locomotion, like the nervous mechanism of respiration, is fundamentally an automatic mechanism. Later on afferent neurones are added to it, comparable to those of the pulmonary branches of the vagus. In this connection it is most significant that in general the same conditions so frequently referred to as stimuli of the respiratory center—lack of oxygen, excess of carbon dioxid, etc.—are the very conditions found to favor the movements of narcosis progression.

If, then, to the respiratory center, which has thus far stood in lonely glory as the one fully established example of automatic nervous action, we must add the fundamental centers of locomotion, the thought at once suggests itself that renewed investigation may find the same thing true of other actions which in the past we have too complacently catalogued under the head of reflexes. The field thus opened up is a large one.

AXON REFLEXES

Text-books of physiology usually record two observations, one by Langley, the other by Bayliss, which were not suspected of bearing any relation to each other and both of which have been difficult to fit into the orthodox scheme of nervous action. So far is this true that Langley's axon reflex has been relegated to the inglorious place of a laboratory curiosity which plays no rôle in normal life, while Bayliss's proposed theory of antidromic impulses has been treated with a polite but uncompromising skepticism.

The axon reflex is a reaction made possible by the branching which generally takes place at the end of an axon. Inasmuch as nerve fibers can conduct impulses in both directions, it follows that stimulation of one of the terminal branches will start an impulse traveling up to the point of union of the two branches, and then down the other branch to the end organ. The axons to a frog's sartorius, for example, branch soon after entering the muscle and it often happens that one branch will go to one side of the muscle, while another branch of the same neurone will pass to the opposite side. If, now, the lower third of the muscle be divided longitudinally, it is found that a stimulus applied to one half so as to excite its nerve fibers will cause contraction of the opposite half of the muscle. The same thing is rendered possible whenever a preganglionic efferent neurone passes through several sympathetic ganglia, giving off collaterals to postganglionic neurones in successive ganglia; in this case stimulation of the terminal branch of the preganglionic neurone will start an impulse centripetally and excite, through the collaterals, the cells with which these collaterals are connected. It is also well known that one must be on his guard against axon reflexes in testing the regeneration of nerve fibers, for it often happens that in the process of regeneration an axon of the central stump may branch before entering the peripheral stump; if these two axon branches find their way into different branches of the peripheral nerve trunk, stimulation of one of these branches may give an apparent reflex, which, however, is only an axon reflex.

These and other examples that may be cited are, however, only laboratory curiosities. Where the two branches of the axon end in a muscle or a gland neither branch can be stimulated at its ending except by

artificial means. If, on the other hand, the same axon should send one branch to a sense organ and another to a muscle, or gland, or blood vessel, we would have the possibility of an axon reflex as a normal event. Recent work suggests that this possibility may be realized.

In 1901 Bayliss[4] found that stimulation of the posterior roots of the sacral nerves between the ganglion and the cord produces dilation of the blood-vessels of the hind limb. The natural explanation of the result, that certain vaso-dilator neurones may send their axons out by the unusual path of the posterior instead of the anterior nerve roots, was disproved by the fact that if the posterior roots are cut near the cord and degeneration allowed to occur, stimulation of the peripheral stump of the cut root still produces the dilation. In other words, the ganglion of the posterior root is the trophic center for some of the essential fibers concerned. From consideration of the known histological possibilities Bayliss concluded that the fibers producing the dilation are the ordinary afferent fibers from the pear-shaped cells of the ganglion, the distal axon being supposed to branch at its ending, one branch going to the sense organ, and the other to the blood-vessel. He furthermore supposed that in addition to serving as a trophic center for the afferent fiber, these cells may be reflexly stimulated by other afferent fibers and thus discharge "antidromic" impulses to the periphery; such impulses passing over the branch to the blood-vessel produces the dilation, while the impulse over the branch to the sense organ would be without effect ("law of irreciprocal conduction"). Physiologists have, however, looked askance on this conception of antidromic impulses, even as a working hypothesis.

[4] Bayliss, *Journal of Physiology*, 1901, XXVI., 173; *ib.*, 1902, XXVIII., 276.

The very important experiments of Bruce,[5] however, put the matter in a new light. It is well known that when an irritant is applied to the skin, a dilation of the arterioles (active congestion) ushers in the inflammatory reaction. Bruce shows that this will not occur if the area to which the irritant is applied is first rendered anesthetic with cocaine. It will occur immediately after section of the anterior roots or of the posterior roots, either centrally or distally of the ganglion; hence it is not a reflex through the cord or the ganglion. It will not occur, however, after the completion of the peripheral degeneration consequent upon section of the posterior root distally to the ganglion. In other words, it would seem to depend solely upon the integrity of the distal limb of the neurones of the posterior roots, and to be independent of any nerve cell whatever. This would seem almost to force[6] the conclusion that we are dealing with an axon reflex. The posterior root fiber branches, as Bayliss supposed, at its ending, one branch going to the sense organ while the other serves as a dilator of the arterioles. The same fiber, probably one of pain, which carries the afferent impulse giving rise to the sensation of irritation produces also the active congestion of the region through its vascular collaterals.

If these observations prove well founded, the axon reflex becomes a reality in the normal functioning of the organism, instead of a laboratory curiosity. Moreover, the facts discovered by Bayliss receive their

[5] A. Ninian Bruce, *Quarterly Journal of Exp. Physiology*, 1913, VI., p. 339.

[6] The writer can imagine only one other possibility; namely, collaterals given off from afferent fibers distally to the ganglion may enter the sympathetic ganglia, which would thus serve as reflex centers. No such collateral communications in nerve trunks have, however, been described. Indeed it is the usual teaching that nerve fibers branch only in the ganglion or at their endings.

ready explanation without the help of the hypothesis of antidromic nerve impulses. Furthermore, if the axon reflex is the foundation of this inflammatory reaction, we may well investigate other reactions which in the past have been classed as reflexes, but upon inadequate evidence. So long as the reflex was supposed to be the only means by which stimulation of a peripheral sense organ can evoke a non-volitional reaction in another organ, all such reactions have been classed as reflexes, and this quite frequently without experimental proof.

CONDITIONED AND UNCONDITIONED REFLEXES

The main purpose of this paper is to emphasize neglected aspects of nervous action. Hence our treatment of reflexes, properly so-called, will be confined to emphasizing the fact that we probably include in the category of the reflex two entirely different kinds of nervous reaction.

It is an interesting fact that so common a phenomena as reflex action is somewhat loosely defined in our thinking. One will call it an action brought about by the stimulation of efferent neurones by one or more afferent neurones; another will add to this, "without the intervention of the will"; another will add, "without the causal intervention of consciousness"; while still another will add, "without the causal intervention of consciousness or the will." These four definitions are by no means identical, as I hope to show. All of them have in common the fact of stimulation of efferent by afferent neurones, stimulation being supposed to include both excitation and inhibition, and it being assumed that any number of intermediate neurones (first, second and third order, etc.) may be interposed between the afferent and the efferent nerves concerned. All would exclude the will from any causal connection with the reaction, and this leaves as the chief point of difference in the above definitions the question whether we should exclude from the category of reflex action all cases where the nervous processes concerned in consciousness play, or seem to play, a causal rôle in the chain of events; for there are nervous actions which are in no sense volitional; which have an afferent and an efferent side, and hence resemble reflexes; but in which we encounter the nervous actions concerned with consciousness. The example which at once occurs to us is the so-called psychic secretion of saliva and gastric juice. To these we would add the no less striking case, brought to our attention by Cannon, of the stimulation of the secretion of adrenalin as the result of the major emotions of fear and anger. In all these cases there is the absence of conscious intention; indeed, the subject is unaware that the act of secretion is taking place; and yet the conscious process is the starting point of the efferent discharge. Shall we or shall we not call such actions reflex actions?

The answer to this question is, of course, entirely a matter of arbitrary definition. If we exclude the causal interposition of consciousness[7] from the reflex, such reactions are not reflexes; if we do not exclude it, they are. The decision in such an arbitrary matter, moreover, is determined on purely utilitarian grounds. Definitions exist only to insure clear thinking by keeping separate and distinct those things having some fundamental point of difference. Thus many would have us believe that there is no really fundamental difference between reflex and volitional acts; that the efferent discharge in the simplest reflex is accompanied by a momentary flash of something that corresponds to conscious intention;

[7] To avoid a cumbersome expression, the word "consciousness" is frequently used for the "nervous events connected with the state of consciousness."

and that there are all gradations between this and the highest development of the human will. Perhaps they are right; we will not argue the point; but we nevertheless retain our two categories of reflex and volitional actions, because so long as this conception of volition is a pure hypothesis it is unwise to forget that what are subjectively different may be objectively different as well.

The same principle of definition should be applied to the case under discussion. A reflex from which consciousness is entirely absent and one in which consciousness seems subjectively to play a causal rôle may, from the objective standpoint, be one and the same thing; and yet so long as this is only one of two opposing tenable hypotheses, it would seem to be the sensible thing to make a distinction between them.

One of the world's most eminent physiologists does indeed make such a distinction. I refer to Pawlow's differentiation between the unconditioned and the conditioned reflex. I can hardly think that Pawlow's very striking experiments upon which he bases this distinction are unknown to physiologists generally and all will agree that he is a man whose opinion should command attention; yet I find no notice whatever of this matter in the three admirable text-books of physiology which are most widely used to-day in England and America. Because of this and in view of the fact that this address is to a semi-popular audience I shall go into this matter at somewhat greater length than if I were speaking to specialists in neurology.

The distinction between the conditioned and the unconditioned reflex is well illustrated by the excitation of salivary secretion through the nervous system. When the taste endings are excited by food in the mouth, a purely reflex flow of saliva results. The work of Miller[8] seems to establish the existence of definite bulbar centers for this reaction, the gustatory fibers of the lingual and the glossopharyngeal nerves serving as the afferent neurones. On the other hand, the mouth may water "at the very sight of food." Here the afferent stimulus comes through the optic nerve, but it differs from that through the gustatory fibers in the fact that the reaction is secured only in a conscious animal. It is also more capricious in its occurrence; the whole setting of the nervous system must be right to have it occur at all; the subject must be hungry, the food must be appetizing, it is more apt to occur at the accustomed hour for meals. In short, a certain state of consciousness must exist to insure effective connection between the afferent optic neurones and the secretory efferent neurones. In the unconditioned reflex the nervous processes concerned in consciousness are in no way involved; it will take place in a decerebrate animal and may occur under anesthesia; it is a rare thing that the application of the proper stimulus fails to elicit it, although, like any other reflex, it may be inhibited, as in the old rice test. Above all, it is not easily lost by disuse, perhaps never permanently lost except by some actual atrophy of the neurones involved.

In a remarkable series of experiments Pawlow[9] actually developed in animals conditioned reflexes which could by no possibility have formed part of the previous life of the animal or of its ancestors. Every time a dog was fed, a piece of ice was applied to a certain part of its skin. In the

[8] F. R. Miller, *Quar. Jour. Experimental Physiology*, 1913, VI., 57.

[9] Pawlow, I. P., Huxley Lecture for 1906, *British Medical Journal*, 1906, Vol. II., p. 871; *Lancet*, 1906, Vol. 171, p. 911; SCIENCE, 1906, N. S., XXIV., p. 613; see also Pawlow's articles on the same subject, *Ergebnisse der Physiologie*, 1904, III., 1, p. 177; *ib.*, 1911, XI., p. 345.

course of time (ten days or two weeks) the application of the ice to the same cutaneous area would evoke a flow of saliva without the formality of feeding. The application of ice to other parts of the skin was also effectual, apparently because the sensations of cold concerned in the result were not local, but more or less generalized. In another series of experiments a note of a certain pitch always accompanied the taking of food, and this stimulus, too, after sufficient repetition, could evoke the flow of saliva, while a note of distinctly different pitch was ineffectual. The reactions thus acquired were soon lost with disuse, although it is possible that if the "training" had been continued over much longer periods of time the reactions might have become more firmly fixed; it is even conceivable that they may take place in the absence of consciousness; that is to say, without the participation of cerebral centers; but these are questions which, so far as I am aware, experiment has not yet answered. Finally, they are more or less capricious; not infrequently the acquired response to the stimulus does not occur, thus contrasting with the response to gustatory stimulation, which seldom fails.

In what way is this type of reaction acquired? The phenomenon of reenforcement (of the knee jerk, for example) shows that activity of any one part of the nervous system causes the irradiation over the entire brain and cord of some exciting influence which, though itself minimal or even subminimal, yet adds itself to any other stimulus that may enter about the same time. Pawlow's work seems to show, moreover, that, when two nerve centers are habitually active at the same time, there is beaten out a path of conduction between the two, the two become "associated" so that activity of the one is liable to excite activity of the other. When, for example, the knee jerk is reenforced by stimulus of sound, not only does such an irradiation from auditory centers pass to all parts of the nervous system, the sacral motor centers included, but one also irradiates from the sacral centers to all parts of the nervous system, the auditory centers included; and just as when there are two lights in a room the path between these lights is the most intensely illuminated portion of the room, so in the case in question the path between the two centers is most strongly in the excited state. If now this same combination of activity be repeated over and over again, this path becomes more irritable and conductive by use until we arrive at the condition shown in the above experiments of Pawlow where activity of one center can, of itself, excite activity of the other. It would indeed be interesting to know whether, just as clapping the piece of ice on the skin evoked a secretion of saliva, so the dog experienced a sensation of cold every time he ate.

The path of conduction or association thus established is presumably through the gray matter, perhaps with the help of the short neurones of the border zones.[10] Our present knowledge of the anatomy of the nervous system is inadequate to give a satisfactory idea of the mechanism involved in the development of this new path of conduction; but it is inconceivable that the anatomical basis of the physiological connection between the centers in question should be the same as that pictured in the typical reflex arc of the text-books. Apart from the improbability of the development of new neurones, the observed facts of the capriciousness of the reaction and the ease with which, once acquired, it is lost by disuse determine as the logical course its provisional classification in a group of its own.

[10] One thinks of the "neuropile" of some histologists as a possible tissue in which this path is blazed.

To look at the matter from another point of view, the present state of our knowledge would seem to indicate that the typical reflex of our text-books, the unconditioned reflex, is a congenital mechanism. The neurones concerned and the collateral connections of afferent and efferent limbs are born with us. It may require practise to bring the mechanism into perfect working order; but practise does not produce a new neurone nor have we any reason for thinking it can produce collateral connections which were not already laid down by heredity. The nervous element in locomotion is a case in point. The colt walks from the moment of birth; a human baby not until its second year, and then only after arduous trial and effort; but this does not mean that the nervous mechanism is congenital in the one case and acquired in the other; it merely means that the congenital nervous mechanism is in complete working order at birth in the colt, while in man either embryological development is not complete until later or else use is required to make congenital synaptic connections efficient. Despite the immemorial antiquity of the expression "learning to walk," it may well be questioned whether any child really learns to walk; whether the facts observed are not equally well explained on the theory that the child finally walks simply because at last the embryological development of its nervous mechanism of locomotion is complete, as is that of the colt at birth; and that the improvement which apparently results from its efforts is in point of fact merely the record of the progress of ontogenetic development.

With learning to talk the case is entirely different. Here there is no inherited mechanism leading to a uniform result in all individuals of the species. One child learns to speak English, another German, another Russian; and if the English child had been taken after the first few months of its life to Russia and heard nothing but Russian, it would have learned to speak Russian as perfectly as it actually learned to speak English while growing up in its native country. In this case heredity has furnished a nervous system capable of acquiring just such associations as those described in Pawlow's experiments; we are dealing with a process in every way comparable to the conditioned reflex.

Finally, if the distinction between conditioned and unconditioned reflexes upon which Pawlow insists is correct, some old statements which take us back to our very introduction to the study of physiology need revision, or at least more accurate restatement. When we speak of "habit being an acquired reflex" we really mean an acquired conditioned reflex. There is no reason for assuming that the reflex acquired by the repetition of volitional acts is the typical reflex arc; indeed there is every reason for believing the contrary. Paths of conduction become blazed between different lower centers because they are simultaneously excited in the volitional execution of an action, and a mechanism is acquired of whose nature we know next to nothing, but through which the act can be performed more and more easily with less and less conscious effort—or, in physiological language, with less and less participation on the part of the higher centers of the cerebrum. We are not concerned with the psychology of this phenomenon, much less is this the place for speculation as to the physiological mechanism involved. We are simply concerned with its classification as a distinct thing from the ordinary unconditioned reflex.

Perhaps when introducing this discussion of reflexes I laid undue emphasis on the rôle of consciousness in the acquisition of conditioned reflexes. In the examples

cited the nervous events associated with the state of consciousness do indeed play a conspicuous rôle. If, however, the essential thing about this reaction is what we have suggested, namely, that the connection between afferent and efferent fibers is a path blazed through the nervous substance rather than a definite localized conduction through specialized neurones, it would seem that consciousness comes so frequently into play merely because it is through the nervous substance of the cerebrum that such paths can be blazed most readily, and the activity of cerebral centers carries with it as a usual thing a state of consciousness. If this be true there is no reason why conditioned reflex associations may not arise between subcortical as well as between cortical centers; it is only necessary that the centers be simultaneously active, reflexly or otherwise; and possibly some cases of associated action of two bulbar or spinal centers—respiratory and vaso-motor, or respiratory and cardio-inhibitory—may be of this kind rather than distinct collateral connections between the neurones of the two centers. This is, of course, only a surmise, but it is clearly a possibility and certainly there is no evidence whatever to exclude it. We have been too quick to assume that coordinations are always effected by the same mechanism, and that too the kind of mechanism pictured in our typical reflex arc. An unproved assumption; and so long as it is an unproved assumption it is the logical thing to keep in separate categories the two classes of reactions which to-day are almost universally thought of as one and the same.

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University of Virginia

THE DEDICATION OF THE NEW BUILDING OF THE MELLON INSTITUTE

The new $350,000 building which will form the permanent home of the Mellon Institute of Industrial Research and School of Specific Industries of the University of Pittsburgh, was formally dedicated on February 26. This building, the gift of Messrs. Andrew William and Richard Beatty Mellon, of Pittsburgh, was especially designed for the needs of the institute; it is distinctly modern in every respect, and complete facilities are provided for the investigation of manufacturing problems and for conducting industrial research according to the practical system of cooperation between science and industry, founded by the late director of the institute, Dr. Robert Kennedy Duncan. By this system, an industrialist having a problem requiring solution may become the donor of a fellowship by providing the salary of the researcher selected to carry out the investigation desired, the institute supplying every facility for the work—laboratory space, the necessary apparatus and supplies, library facilities and advice of a staff expert in industrial research, etc.

The new home of the Mellon Institute is a five-story and attic building. The basement contains seven rooms: the main storeroom, the boiler room, the electric furnace room, a heavy apparatus room, a room equipped for low-temperature work, the machine shop and a kitchen. On the first, the main floor, are located the general office, the director's suite, the office of the editorial department, the library, the office and laboratory of the assistant directors, the assembly hall, a special apparatus room and a dark-room laboratory. The second and third floors each contain ten large research laboratories and nine small ones; the fourth floor, which is not finished, will contain an identical number of laboratories as soon as the growth of the institute warrants its completion.[1] At the present time twenty-three fellowships are in operation and forty research chemists are engaged in a study of the variety of industrial problems under investigation at the institute.

While the Mellon Institute possesses an endowment of its own and has its own board of trustees, it is an integral part of the Univer-

[1] For a full description of the new building of the Mellon Institute, see *The Journal of Industrial and Engineering Chemistry* for April, 1915.